As this annual increment is at the best small and as for ten years there has been a steady loss, it seems apparent enough that the number that might be taken with safety has been very much exceeded.

It is conceded that pelagic sealing has of late fallen off in a greater ratio than the herd, thus producing a tendency toward equilibrium in numbers. This simply means that over 60,000 seals were taken in 1895; 43,000 in 1896, and 26,000 in 1897, so that the pelagic catch has fallen off one-half in three years, although the herd has not diminished by one-half in the same time.

It is to be feared that before any equilibrium could be reached but a small portion of even the present number would be left, and this leads naturally to the next point agreed upon, which is that in estimating the future conditions of the herd the reduction in the number of pups caused by the pelagic catches of 1894 and 1895 must be taken into consideration.

For example, not less than 20,000 pups, half of them females, perished of starvation in 1895, owing to the death of their mothers from pelagic sealing. Not only did the portion of this number that would have survived fail to appear on the rookeries in 1897, but the number of births will be naturally lessened by just that number in 1898 and the progeny of these in turn fail to appear in 1900. Thus, as the natural decrease will go on, while the natural increase has been cut off, effects of pelagic sealing will be felt up to 1900, even should it be stopped at once.

The final conclusion is that the herd is not in danger of actual extermination so long as its haunts on land are protected and the protected zone about the islands is maintained, and that both land and sea killing now yield an inconsiderable profit. The seal herd is in fact very far from actual extermination, although the point of commercial extermination, or that where the returns are wholly incommensurate with the amount of capital invested, has been nearly reached. But for the prompt action of the United States in 1869 this point would have been reached years ago, while but for its care of the islands ever since practical extermination would not be far off.

The example of the Southern fur-seal illustrates the rapidity with which commercial extermination may be effected, while the fate of the fur seals on the Farallones, Guadalupe and Juan Fernandez shows how readily actual extermination may take place. The Pribilof Islands are not, like those of the Antarctic, difficult of access, and their abandonment by this government would lead to the actual extirpation of the fur seals within a very few years. On the other hand, with proper protection the fur-seal herd can, with but little care and cost, be made an important source of revenue so long as fashion may decree the wearing of of seal-skin sacques.

THE PSYCHOLOGY OF THE PERSONAL EQUATION.

In the present paper the writer proposes to maintain the thesis that the personal equations of astronomers are mainly controlled by known laws of experimental psychology and hopes to assist his professional brethren in making use of the researches of the psychologists in such a manner that they shall avoid groundless hypotheses

which have often been harmful by leading to premature conclusions and thus have hindered the progress of our knowledge.

I do not wish to be considered as denying the possibility that the psychologists may learn something from the steady and well understood routine of the astronomers in their practical operations. In truth, almost the first step towards the measurement of the times occupied by mental processes was taken by the great astronomer Bessel in his classical memoir on 'Personal Equation,' which is reprinted in the third volume of Engelmann's edition of his 'Abhandlungen.' Bessel indicated there very clearly that the cause of the riddle which he partially solved was a psychical one, namely, the impossibility of comparing the impressions on two senses, sight and hearing, which take place exactly at the same instant. The 'eye and ear' observer who uses Bradley's method as modified by Maskelyne in noting down the time when a star image passes a fixed thread in his telescope makes the attempt to combine these two impressions, and in so doing necessarily 'apperceives' one of them before the other, and this produces 'personal equation,' or constant error in the time, which in seconds and fractions he notes down in his observing book. Bradley, in his journals of observation, noted vulgar fractions of the seconds. Maskelyne, Bradley's successor, failed to discover even the facts of personal equation, except in the work of one of his assistants. This case, that of Kinnebrook in 1795, was apparently settled by discharging the poor fellow as irregular and 'vitious' in his methods of observing. The personal equations of Maskelyne's other assistants were not known till many years later; Bessel's discovery, made about 1820, originated in his reading Maskelyne's notes about Kinnebrook's apparent want of skill. After the publication of Bessel's memoir the matter was followed up by W. Struve, Airy and other eminent astronomers, before the invention of the chronograph in 1849 had become the common property of astronomers. It led very soon to the thought that something similar had place in chronographic registration. This suspicion was fully confirmed before or soon after the 'American Method' was introduced at Greenwich.

In 1861 the psychological side of the investigation was taken up by Professor Wundt and led to the important discovery of the displacement of time when the observer attempts to fix the place on a divided scale of a continuously moving object. This displacement of time can lie in either direction, and thus an apparent difficulty is relieved which arises from the facts of personal equation as observed by Bessel, W. Struve and Argelander, and also by the astronomers who took part in Struve's chronometric expeditions of 1843 and 1844. The regular Greenwich investigations began with Airy's entrance upon the office of Astronomer Royal, in 1835, and have been continued to the present day.

The attempts by means of personal equation machines to determine the absolute personal equations of eye and ear observers led to the general result that most observers anticipate the true time of the transit, and that those who, like Argelander, fall behind it are fewer in number. If we take the average observer as the standard, as Sir George Airy seems to have intended, we find but few of those tested between 1835 and 1859 who fell behind the standard observer for the time, or behind the average of the body of observers in employment together at Greenwich. In my paper in the *Monthly Notices* of the Royal Astronomical Society for May, 1897, I have shown that the Greenwich observers since 1885 have anticipated their own times of chronographic transit by an amount not far from $0^s.13$, on the average. One excellent observer,

Mr. Hollis, anticipates his own chronographic transit by $0^s.5$, and does this, not because his eye and ear anticipation of the true time is very large, but because he employs the slower method of registration, first pointed out, if I mistake not, by Le Verrier, and his chronographic transits are about $0^s.2$ later than those of the average observer. The difference $0^s.13$ between the average observer's eye and ear transits and chronographic is about the average time of a 'simple reaction' according to the psychologists, and thus its amount confirms Wundt's view that chronographic registration by a practiced observer is closely analogous to the process of simple reaction. We thus reach the conclusion that the anticipation of such an average observer, added to the positive time which he requires to make a registration, is but $0^s.13$, and a partial compensation would take place if the two methods were used in connection. The best form of personal equation machine is, as it appears, the Repsold Transit Micrometer. The tests which have been made of it, as of other contrivances for a similar end, are not entirely conclusive for various reasons, and the chief of them in this case seem to arise from the novel construction of the instrument and the short experience of the observers with it.

I quote from the *Astronomische Nachrichten*, No. 3,036, the following differences chronograph-Repsold Micrometer for Professor Becker and his colleagues :

	Chronograph-Repsold Micrometer.	Mean Error.
Becker	$+0^s.319$	$\pm 0^s.009$
Halm	0.224	0.009
Kobold	0.110	0.011
Zwink	0.158	0.016

These personal equations are of the same general order of magnitude as the reaction times found by a multitude of psychological experimenters. The largest differences among them may without much danger be ascribed to the same cause as the chronographic personal equation between Mr. Lewis, now standard observer at Greenwich, who employs the quicker method of registering, and Mr. Hollis, who employs the slower; their average differences for ten years is $0^s.24$, varying very little from year to year, and that between Dr. Kobold and Professor Becker is $0^s.209$ by the data given above and $0^s.255$ by direct chronographic registration. The former registers rapidly and the latter slowly, but they do not give in the article cited any account of their psychical process.

Wundt, in his classical work 'Grundzüge der physiologischen Psychologie,' expresses the decided opinion that astronomers will do well to study the methods of the psychologists in dealing with personal equations, and this conviction I venture to consider as confirmed in a definite numerical way in my paper in the *Monthly Notices* to which I have referred. Additional arguments in favor of the psychological conclusion are not wanting and will be briefly stated. In the first place eye and ear observers note the times of transits differently when they observe stars on different sides of the zenith. There does not appear to be any certain general rule as to the sign of this form of equation. Chronographic observers register transits of time stars about $0^s.02$ or $0^s.03$ later when the direction of motion is opposite to the usual one. This agrees with the psychical law that unfamiliar circumstances tend to delay reaction. Faint stars are observed by chronograph later than brighter ones. This delay, about $0^s.013$ per magnitude, has been tested by several of the most eminent observers now living and found to increase in amount as the stars approach that magnitude at which they are observed with difficulty by a given transit instrument. A similar delay in reaction to a sense impression, not a linear function of the intensity of the sense

impression, is found to be shown when the sense impression is faint. Even the amount $0^s.013$ per magnitude is approximately indicated by the experiments of G. O. Berger and Professor Cattell, who have found, for a diminution in intensity to one-thousandth of its normal amount, a delay in the reaction to impressions on the sense of sight by $0^s.113$ for himself as reagent and $0^s.108$ for Dr. Cattell. The diminution in intensity corresponds to seven and one-half magnitudes on the usual astronomical scale: $7\frac{1}{2}=\log. 1000 \div 0.40$ and the quotients $0^s.113 \div 7\frac{1}{2}$ and $0^s.108 \div 7\frac{1}{2}$ are approximately $0^s.013$.

The eye and ear transits of very faint stars are liable to singular variations of personal equation; sometimes they are noted relatively too late and sometimes too early. Argelander suspected this last variation for himself and Auwers has proved it, and Argelander's explanation fits the phenomena as observed. He considers that according to Bessel's theory he 'first saw' and 'then heard,' but that when the star was very faint this displacement of time was reversed in direction. For stars not below the limit of magnitude where observation is easy this reversal did not take place. Auwers has shown that in similar cases Bauschinger observed very faint stars too soon by about one-tenth of a second of time. The reversal in Argelander's case produced a variation a good deal larger for extremely faint stars than Bauschinger's, but for either observer the fact of reversal is highly probable. In my paper before cited I give only a part of the details of the Greenwich observers' two-method personal equations. Those details which I do give indicate very clearly that the two-method personal equation varies from year to year, as had been previously found by Dr. Hilfiker, A.N., No. 2815. For example, Mr. Crommelin anticipated his own chronographic transits by $0^s.20$ in 1892 and by $0^s.10$ in 1893. Mr. Bryant by 0.07 in 1892 and $-0^s.01$ in 1893. Similar variability from year to year is indicated in the work of the veteran observers, H.T., A.D., T.L., and H., for whom only average values are given in my paper. Nor are such alterations of personal equation confined to the eye and ear personal equation or its difference from the chronographic. Professor Turner, Chief Assistant, anticipated Mr. Downing with the chronograph by $0^s.15$ in 1886 and by $0^s.08$ in 1888. Mr. Lewis, now 'standard observer,' anticipated Mr. Downing by $0^s.01$ in 1886 and $0^s.10$ in 1891, and similar changes are visible in the eye and ear personal equations relative to the standard observer of the year. The instrument and stars were essentially the same for all the years between 1885 and 1893, so that the changes in the two-method personal equations are psychical variations in the habits and methods of registration and of noting eye and ear transits. The study of these habits and methods is plainly a branch of experimental psychology. We have already obtained some indications of how astronomers can promote this branch of study to their professional benefit as well as to that of psychological science. The simplest way for them to proceed is first of all to make more extensive comparisons of the eye and ear and chronographic method than have already been accomplished. Such comparison of eye and ear and chronographic transits of stars of various magnitudes would with little trouble extend our knowledge of the subject to a considerable degree. The Greenwich astronomers have detected the 'Gill equation' in chronographic transits; especially with the altazimuth. The same form of personal equation has been found in Professor Küstner's Berlin observations and by Mr. Tucker, of the Lick observatory, in his own. The 'Helligkeitsgleichung' for chronographic transits is, as before stated, already well

known and exhibits some uniformity. The great observatories which still so effectively employ the eye and ear method have a wide field of investigation open to them for the places of unknown stars which are not too faint to be more advantageously observed by eye and ear than by chronograph. Of course, immediate micrometrical measurement would give us enough material for completely settling the 'Helligkeitsgleichung' for most living astronomers. Stars within five degrees of either pole can now be observed by the Greenwich method, and the doubts and perplexities which hinder the accurate study of their right ascensions will soon vanish.

The Repsold micrometer, already an indispensable tool in determinations of longitude, is not well enough known to enable the observers to omit the exchange of their own stations and instruments.

But the numbers obtained by its use cannot, I suppose, yet be called absolute personal equations, but from them I venture to guess that the difference between the largest 0.319 for ProfessorBecker and the smallest 0.110 for Dr. Kobold implies that Professor Becker employs the sensorial or deliberate form of registration and Dr. Kobold the motor or shortened form ; as the difference between them, $0^s.209$ with a mean error of 0.014, is nearly equal to that of $0^s.24$ between Messrs. Lewis and Hollis at Greenwich from eleven years' comparisons, 1883–1893.

The conclusion which some astronomers appear to favor, that by Bradley's method, as well as by the chronographic, faint stars are observed relatively later than brighter ones, is shown not to be a safe one, because we have cited two examples of thoroughly skilful observers who observe very faint stars earlier than those whose observation by eye and ear is easiest and most accurate. There is also a series of 'Coefficients of the light equation collected by Professor Auwers in his Berlin Zone of the A. G. C.,' in which there is so little uniformity even as to sign that he is led to remark upon the uncertain and fragmentary nature of the results in this matter ; so far as obtained by astronomers. If the eye and ear observer is careful not to observe transits of stars too faint to be seen in the illuminated field, we are partially justified in concluding with Bauschinger that the 'Helligkeitsgleichung' is not then a sensible form of personal equation for eye and ear work, and we can also conclude that it deserves attention chiefly in its psychical aspect and that the method of making good eye and ear observations of transit is to make sure by ingrained habit of the uniformity of the psychical process from star to star, especially when the attempt is made to determine the right ascension of faint stars like the fainter ones of the 'Durchmusterung.'

The writer is not aware that any other astronomer has made the attempt to compare the numerical results of the psychologists with the personal equations as determined in the usual routine of observatory work, and is inclined to recommend the continuance of such comparisons in directions which at once suggest themselves.

First. Comparisons of eye and ear and chronographic transits of stars in various magnitudes and declinations.

Second. Comparisons of observations by both methods with the Repsold Transit Micrometer.

The subject of this paper is so broad and goes so deeply into many problems of practical astronomy where high accuracy is aimed at that the writer can only express regret that his attempts to deal with the matter have been so inadequate, and hopes that at a future time he may be able to prosecute it farther by the kind assistance of his colleagues and pupils.

TRUMAN HENRY SAFFORD.

WILLIAMS COLLEGE,
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